Author's response to reviews

Title: Persistent socioeconomic inequalities in cardiovascular risk factors in England over 1994-2008: a time-trend analysis of repeated cross-sectional data

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Author's response to reviews: see over
Dear Victorino Silvestre and Prof Young-Ho Khang

MS: 5438487145395786
Shaun Scholes, Madhavi Bajekal, Hande Love, Nathaniel Hawkins, Rosalind Raine, Martin O’Flaherty and Simon Capewell

Thank you very much for inviting a revised manuscript.

We attach, as suggested, a point-by-point response to the issues raised.

We are very grateful for your constructive comments and have incorporated them into the revised manuscript. All amendments are shown as “underlines” to aid visibility.

We hope this manuscript is now acceptable for publication in BMC Public Health

With Best Wishes

Yours Sincerely

Shaun Scholes, Madhavi Bajekal, Hande Love, Nathaniel Hawkins, Rosalind Raine, Martin O’Flaherty and Simon Capewell
RESPONSES TO REVIEWERS

MS:  5438487145395786
Shaun Scholes, Madhavi Bajekal, Hande Love, Nathaniel Hawkins, Rosalind Raine, Martin O’Flaherty and Simon Capewell

Reviewer 1: Kyunghee Jung-Choi

Major compulsory revisions
The purpose of this paper was to assess socioeconomic trends in cardiovascular risk factors in England between 1994-2008. It is important and interesting to explore the effect of the mediators between socioeconomic position indicators and health outcomes such as CVD. Although the study has been well conducted, I think that some issues of the paper can be improved.

Thank you.

1. In the title, the main focus of this manuscript is thought to be ‘the trends in socioeconomic inequalities’ in cardiovascular risk factors rather than ‘the trends in cardiovascular risk factors’. Although the authors tried to explain the trends in cardiovascular risk factors by deprivation quintiles, table 2 and table 3 seemed not to give a clear message about changes in socioeconomic inequalities. I think that is mostly because the time period considered in table 2-3 (the trends in cardiovascular risk factors) is different from figure 2 (the trends in socioeconomic inequalities in cardiovascular risk factors).

The authors have two options: to divide the trends in cardiovascular risk factors into the 1990s and 2000s like RII trends, or to present RII’s in 1994 and 2008 respectively. My advice is that they follow the first option because, in the discussion section, the authors noted the problem of sample size. Four tables by gender and age included annual percentage change and RII by deprivation quintile in the 1990s and 2000s can be presented. More information than from just figures can be included in this way, and then annual percentage change in cardiovascular risk factors may have more meaning as an explanation of change in inequalities.

Thank you. All four reviewers requested more emphasis be placed on the monitoring of change in social inequalities in risk factors – rather than on trends in risk factors by socioeconomic group. In addition we were recommended not to use the Relative Index of Inequality and to present trends using annual data rather than split the Health Survey for England into two discrete time periods (see Reviewer 4). We have taken this very good advice. We have given a great deal of thought about how to best present the information in a succinct manner. In the rest of the document we set out the changes we have made.
2. In the discussion part, the authors report the explanations for trends in cardiovascular risk factors. Explanations for trends in socioeconomic inequalities in cardiovascular risk factors are needed. Although the authors describe the results of other studies, it is insufficient to explain the changes in England. More clarification is needed in the meaning and reasons for changes in England.

Thank you. Expanded text on the meaning and reasons for overall risk factor trends and changes (or lack of change) in social inequalities in risk factors in England has been added to the discussion section on Pages 20-22. Included in the discussion are references to a number of policy initiatives including salt policy (see comment below). Due to limitations of space we have deleted references to international studies from the discussion.

Minor essential revisions (Minor issues not for publication)
1. Table1. Regarding ‘physical activity’, the year 1994 was not shown, which is different from the text.

Thank you. The text on Page 6 has now been amended:

“The physical activity questionnaire used in 1994 was not comparable to that used in later years and so our start year for estimating trends was 1998.”

2. Discussion, Explanations for trends, first paragraph. The authors suggested the explanations for declines in smoking, SBP, and cholesterol included lifestyle changes such as alcohol consumption and weight loss. The references of the study in England are needed for changes in alcohol consumption and weight loss. (Moreover, the author’s explanation of the favourable changes in weight loss contradicts the result)

Thank you. We have removed reference to weight loss as a potential explanation for risk factor changes. Due to limitations of space, we have focused attention on presenting definitive evidence on changes in the seven cardiovascular risk factors and so have excluded reference to changes in alcohol consumption as an explanation for risk factor trends.

A systematic review of recent evidence on trends in alcohol consumption presents mixed evidence (trends vary by choice of indicator) and there is a lack of evidence on changes in socioeconomic inequalities in alcohol consumption. “Smith L, Foxcroft D: Drinking in the UK: An exploration of trends. Joseph Rowntree Foundation; 2009”. We have not included this reference in the manuscript.


Thank you. Expanded text on the meaning and reasons for overall risk factor trends and changes (or lack of change) in social inequalities in risk factors in England has been added to the discussion section on Pages 20-22. We discuss salt policy in England on Page 25:
“Recent declines in blood pressure reflect changes in health behaviours (diet e.g. through lower salt intake, physical activity and tobacco consumption), together with wider use of antihypertensive medication [37]. Salt intake levels have reduced by almost 1 g/day over 2001-08 in people aged 19-64 years, reflecting voluntary agreements with the food industry to reduce the salt content of processed foods, plus health promotional initiatives [38-40].”

4. Discussion, Explanations for trends, last paragraph. The references of the study in England are needed as well.

Thank you. All relevant references for risk factor trends and changes in inequalities in England have been made.

5. Abstract, Conclusions. Perspective more focused on the main issue (inequalities in cardiovascular risk factors) is needed.

Thank you. In response to all four reviewers we have revised the manuscript to place a central focus on the size, direction and changes in socioeconomic inequalities in risk factors. Both the abstract and conclusions (Pages 23-24) have been revised accordingly.
Reviewer 2: Enrique Regidor

This paper cannot be considered a scientific paper. I think not be considered a monitoring report on health inequalities either. The reader is lost among the large number of tables that provide the authors. But this is not the main limitation of the paper. The limitations of the paper are several.

For example, in the introduction authors does not adequately justify why it is important monitoring inequalities in cardiovascular risk factors in England. They offer much information, but they cannot convince the reader of the importance of the paper. On the other hand, they make statements whose significance is not well understood. In the second paragraph, they say that the narrowing of absolute Inequalities in CHD death rates was accompanied by widening relative inequalities in mortality because mortality rates decline more slowly in most deprived areas. But that makes sense. What is so special!

Thank you. We have amended the text on Page 4 as follows:

“In England, narrowing of absolute inequalities in age adjusted CHD death rates from 1982 to 2006 coincided with slower relative rates of improvement in the most deprived areas [8].”

In the introduction (Pages 4-5) we more clearly set out the reasons why it is important to monitor changes in social inequalities in risk factors:

“Although downward trends in CHD mortality have been impressive, the slower relative rates of improvement in the most deprived quintiles show that the gains could have been larger than those observed had the gains been shared equally across all areas. Given the importance of risk factors in explaining population trends in CHD, it stands to reason that any change in the magnitude and/or direction of socioeconomic gradients in CHD mortality may be explained by parallel changes in risk factors [9]. However, evidence on changes in social inequalities in risk factors in England is limited. A prospective cohort study over a twenty year period assessed major risk factors only twice and did not include women or older men [10]. Yet monitoring the magnitude, direction and change in risk factors by social groups in the adult population as a whole may have powerful implications for present and future inequalities in CHD mortality.

Using data from the Health Survey for England (HSfE), we assessed the pace of change in seven cardiovascular risk factors by age, gender, and socioeconomic groups from 1994 to 2008 and monitored changes in absolute and relative inequalities. Both measures are essential: using relative measures alone fails to allow monitoring of changes in overall population health or changes in risk factor levels across groups [11]. Furthermore, the size, direction and change in measures of inequality are associated with underlying levels of health. Relative inequalities tend to be larger when prevalence is low, whereas inequalities measured on an absolute scale are negligible at both very low and very high levels [12-13]. If levels of risk factor exposure decline across all groups (i.e. improve over time) declines in absolute inequalities (which are beneficial from the perspective of overall population health), may coincide with increasing inequalities on the relative scale. Guidance from the World Health Organization recommends that monitoring both absolute and relative inequalities is needed to provide a clear picture of health and its distribution across society, and, crucially, to assess policy impacts on health equity [14].”
Regarding methods why authors use the relative index of inequality (RII). They grouped geographical areas in quintiles. The average population in each area is about 1500. Has it changed a lot the size of the population of each quintile to over the years to justify the use of RII? Have authors tested that the trend in the prevalence of risk factors across the quintiles does not deviate from linearity? Why the authors have not monitored the absolute differences?

Thank you. We have taken this very good advice. First, in response to reviewer comments, we no longer use the RII to summarise inequalities as the English population is spread evenly across the deprivation quintiles. Second, we do now test whether the trend in risk factor prevalence deviates from linearity. Third, we estimate the magnitude, direction and change in both absolute and relative inequalities (and explain in the manuscript why both measures are needed for a complete picture). These issues are discussed in the Statistical Methods section (Pages 9-12). For a short description of the revised methods we have used see the responses to Reviewer 4 on Page 14 of this document.

In any case, the results section is the weakest. It makes no sense to provide such amount of data. The authors should have summarized the information presented in a limited number of tables and graphs.

Thank you. We have thought hard about how best to present the results in a succinct summary. We were recommended to move the graphs of risk factor change from supplementary data to the main paper (See Reviewer 4) and move the tables of annual percentage change to supplementary data.

Tables 2 and 3 now present the results for absolute and relative inequalities. The number of Additional files provided as supplementary data has been reduced from 14 to 9. We also make the point in the text that providing measures of absolute change and annual percentage change for risk factors by socioeconomic group assists in explaining the changes in inequalities (See comments by Reviewer 1). Our amended text on Page 22 is as follows:

“Presenting absolute and annual change informs interpretation of changes in inequalities. Absolute and relative measures of inequality were used to provide more complete detail.”

Finally, in the discussion the authors comment on the findings in relation to the objective of the study. The authors discuss the trend in the prevalence of risk factors, but they do not comment on inequality trends in risk factors.

Thank you. We have expanded the discussion (Pages 20-22) to comment on changes in socioeconomic inequalities in risk factors (especially in England). Our revised manuscript now places a central focus on changes in inequalities.
Reviewer 3: Sam B Harper

1. abstract. The estimates given for the RII also need measures of precision so that readers may judge how strong the level of evidence is for changes over time.

Thank you. We have replaced the RII with alternative measures of absolute and relative inequalities (see replies to Reviewer 4 below). In the abstract we provide p-values for the interaction terms testing change in socioeconomic inequalities over time.

2. p4. Was BMI calculated from self-reported or measured height and weight? From Table 1 it appears to be measured in all years, but in the text it is noted that the nurses’ examination did not take place in 1994 and 2004. Measured status would likely reduce bias, but it's not clear to me how this was actually used in the analysis.

Thank you. Height and weight were measured during the interviewer visit and so was available in all survey years. Our amended text on Page 6 is as follows:

“For firstly an interviewer’s visit during which a questionnaire was administered and height and weight were measured.”

3. p5. More should be said about the measurement of deprivation, especially over time. When were the variables in the index measured (all in 2007?), and from what data source? How well does IMD in 2007 measure deprivation in 1994? Is it not possible to obtain measures of deprivation at each survey wave (or at least at one earlier time point)? Why not? Given that this is the primary exposure, more clarity in its measurement would improve the paper.

Thank you. Our amended text on the measurement of deprivation using IMD 2007 (Pages 7-8) is as follows:

“Deprivation indices developed prior to the introduction of IMD such as the Carstairs Index and Townsend Index were based solely on census data and compiled at the electoral ward level of geography. The advantage of using IMD is that it combines census data with other data sources which can be updated regularly over the inter-censal period. Furthermore, it is calculated at LSOA level whose boundaries, unlike electoral wards, remain fixed over time making IMD more suitable for measuring change over time. LSOAs have a mean population of 1,500 people and so are smaller on average than wards (average of 6,000). Using smaller areas increases the likelihood that populations are more homogenous – larger areas such as wards are more likely to group together populations which differ in levels of deprivation [19].

IMD was first introduced in 2004 (based on 2001 data) and has been updated in 2007 (based largely on 2005 data) and, most recently, in 2010 (using 2008 data). IMD scores are compiled using data from the 2001 Census and a variety of sources including from routine administrative returns to government departments (Health, Work and Pensions, HM Revenue & Customs, Children, Schools and Families, Communities and Local Government, Transport, Office for National Statistics) and non-governmental agencies (National Asylum Support
4. p5. Does physical activity refer to leisure-time only or does it also account for activity in the course of work?

Thank you. Our amended text on Page 6 reads as follows:

“Work-based activities were excluded from the summary measure of physical activity.”

5. Table 2. I'm not sure I follow the rationale for using “N/A” in places to identify when quadratic terms are “not significant.” At first glance I thought this meant that the estimates with “N/A” indicated where quadratic terms were not included in the model, and thought it strange that there was a quadratic term nationally for BMI in the 16-54 group but not for any of the quintiles. I think this is potentially confusing to readers, so why not just include the estimate and standard error for the quadratic terms wherever they were fit, and let readers be the judge of whether one should treat a quadratic term with a p-value of .06 exactly the same as a p-value of .99 (i.e., “N/A”).

Thank you. We are reluctant to present estimates for the quadratic terms which were not statistically significant as this could, for some readers, hinder interpretation of the linear trend. A similar issue arises when interaction terms are not statistically significant. For example, a short note on running GLMs in SAS states that “If the interaction term is not statistically significant, some would advise dropping the term and rerunning the model with just the main effects, so that the main effects would have an unambiguous meaning.” (http://www.ats.ucla.edu/stat/sas/output/sas_glm_output.htm).

In the notes to Additional files 4-5 we have amended the text to give a clearer explanation of our approach. In addition, our revised Tables (Additional files 4-5) show the p-value for the non-significant quadratic terms to allow readers to judge the significance of the quadratic term. Our note is as follows:

“A quadratic trend (year^2) indicates a significant but nonlinear trend in the data over time. A linear trend (year) is depicted with a straight line; a quadratic trend as a curve with one bend. Trends that include significant quadratic and linear components demonstrate nonlinear change in addition to an overall increase or decrease over time (and so are not comparable to models just containing a linear term). Two models were fitted to each deprivation quintile. Model 1 contained just the linear trend; Model 2 included linear and quadratic trends. Estimates from Model 2 are shown if the quadratic term was significant at the 1% level. For quadratic terms not significant at the 1% level we show the linear trend from Model 1 - but show the p-value for the non-significant quadratic term obtained from Model 2.

6. p13. While generally I applaud the authors' presentation of the absolute trends underlying their analysis, there are some results that are hard to interpret, or that might be made easier if the tables were altered. The only increase in relative inequality was for SBP>=140 mmHg among young women. However, this is very difficult to judge from the evidence presented in Table 3, where it appears that
the decrease in SBP is considerably greater in Q5 (-.40) than Q1 (-.18), quadratic effects notwithstanding. Okay, fine, then perhaps I'll look at Additional file 12, which has the absolute changes in risk factors for young women. But there we find that the decline in Q1 is from 123 to 116, and in Q5 from 123 to 117. This hardly seems like convincing evidence of widening inequality. I guess I'd say that I don't understand why, if the authors are going to use dichotomous risk factor thresholds to measure relative inequalities, why not present these trends as the primary tables in the paper? As it stands, I can't actually see what the proportion of young women with SBP>=140mmHg in each quintile is over time, which is the main thing driving the inequality results. So, I'd suggest either replacing the mean values of BP, cholesterol, fruit veg, etc. with the proportions over the thresholds and putting these average values in one of the supplementary tables, or simply adding them to Tables 2 and 3. It will make it much easier for readers to get a sense of how the RII estimates relate to the actual values observed for each quintile.

Thank you. We have taken this very good advice. Graphs of risk factor change (Figures 1-4); estimates of absolute change between the first and last available year (Additional Files 4-7); annual change (Additional files 8-9) and estimates of the magnitude, direction and change in socioeconomic inequalities (Tables 2-3) are now presented for each of the seven dichotomous risk factor thresholds. Estimates for continuous risk factors – body mass index, systolic blood pressure, fruit and vegetable consumption (portions) and total cholesterol – are provided in the Supplementary data – but not discussed in the manuscript.

7. One other suggestion would be to make Additional File 14 part of the main manuscript. These estimates are the primary evidence used to make the authors’ conclusions, so why not put them front and center of the manuscript? Again, I say this with appreciation of the authors’ willingness to provide a great deal of additional data and tables, but many readers may not be bothered to consult all of the additional files.

Thank you. We have taken this very good advice and present estimates of the magnitude, direction and change in socioeconomic inequalities in Tables 2-3 at the centre of the manuscript.

8. p13. I worry that the authors are leaving themselves open to arguments about selective citation of evidence. The statement that 5/7 risk factors showed no change is technically incorrect, if I read Additional File 14 correctly. In that table, it is clear the RII for obesity among younger women has decreased, and significantly so (p=0.048), though there is no asterisk on the p-value. Why is this not worth mentioning in summarizing the results? Isn't the fact that inequality in obesity is decreasing a good thing? (it's also worth mentioning in passing that this is largely a consequence of using relative, rather than absolute measures of inequality. If one used absolute measures it is clear from the obesity figures in Additional file 6 and the data in Additional file 12 that absolute inequality is actually increasing). Moreover, the 5/7 is not really the relevant metric, since the authors have made many more comparisons...I'd suggest stating that 25/28 estimates showed no evidence of change, 2 showed increases, and 1 showed a decrease of relative inequality.
Thank you. We have taken this very good advice and present estimates of the magnitude, direction and change in socioeconomic inequalities using both absolute and relative measures of inequality (Tables 2-3). We also present the results of the statistical tests in the metric suggested. Our amended text on Page 15 reads:

“A total of 56 tests of change in inequalities over time are shown in Tables 2 and 3 for men and women respectively. Results for change in absolute inequalities are shown by Models 3a; change in relative inequalities by Models 3b.

Four tests - the prevalence of raised cholesterol in young men and women - showed no change over time, i.e. no association with IMD. No change in inequalities occurred in 42 tests; significant changes were found in 10. First, five tests showed increasing absolute inequalities were in obesity in older men and women, diabetes in young men and older women, and physical activity in older women. Second, relative inequality increased in high blood pressure in young women. Third, four tests for the prevalence of raised cholesterol showed increasing absolute and relative inequalities from 1998 onwards in older men and women.”

9. p14. “the gap” is potentially misleading, as some readers (including this one) would generally interpret “gap” as an absolute measure. Why not just say, “relative inequality widened...”?

Thank you. In the revised manuscript we have avoided using the word “gap” and discuss absolute and relative inequalities as appropriate.

10. p19. The authors make an argument that, whereas high-risk approaches may disproportionately benefit affluent groups, whole-population approaches (e.g., smoke-free legislation, industry regulation) may be more effective at reducing inequalities. Yet on p15 it is precisely the whole-population approaches the authors cite as explanations for the downward trends in cardiovascular risk factors. Thus it seems a little odd to me that, if we take the authors’ results as validly indicating persistent—if not increasing—inequalities in risk factors, that the authors would suggest that whole population approaches are necessary to reduce relative inequalities. Given the arguments on p15 on “Explanations for trends” it would seem that the whole-population approaches the authors cite are precisely the ones maintaining/exacerbating relative inequalities. How can whole-population approaches be both the cause relative inequalities and the solution?

Thank you. Our amended manuscript now focuses on absolute and relative inequalities and as a result is clearer on the messages we wish to give. Taking the lead from recent studies we acknowledge that whole-population approaches may be effective at reducing inequalities on an absolute but not necessarily relative scale. Our amended conclusions read as follows:

“Recent studies suggest that more socioeconomically disadvantaged groups will gain larger benefits, on an absolute scale, if unequally distributed risk factors are reduced proportionally across groups using whole-population based strategies [52-53]. However, although it is possible that policies such as cigarette taxation may particularly benefit more socioeconomically disadvantaged groups, the precise impact of other policies on the
differential reduction of other major risk factors has not yet been established [52]. Furthermore, improvements in absolute but little progress in reducing relative inequalities would still leave groups at lower ends of the social hierarchy at a comparative disadvantage. Therefore, those evidence based population level strategies recommended but not implemented in England (e.g. food labelling, banning industrial transfats and mandatory changes to the food supply to halve the salt content of bread) should be introduced in combination with known effective interventions targeted at those at high-risk to achieve a narrowing of social inequalities.”

11. As a last point, I think the authors, at the very least, need to mention the issue of absolute, rather than relative inequalities. We find in many cases when overall population prevalence declines, relative and absolute inequalities go in different directions. Perhaps more importantly, why are relative, rather than absolute, inequalities the main quantity of interest? At least some defense of this seemingly arbitrary choice should be given.

Thank you. In the revised manuscript we present results for both absolute and relative measures of inequality.
Reviewer 4: Bruna Galobardes

This manuscript investigates the trends in inequalities in cardiovascular risk factors between 1994 and 2008 in England. This is a well written paper in a relevant area of research using data from the Health Survey of England.

Thank you.

I have some comments about specific aspects of the methodology:
1) In page 5, the authors state including risk factors irrespective of medication use because they are not interested in the underlying reason generating the inequalities. Lipid lowering drugs were included in their assessment of total cholesterol (stated in Table 1). People under treatment are still at higher risk of developing cardiovascular disease compared to those who are not, therefore I would argue that to fully account for the inequalities in risk factors treatment should be included in the definition.

Thank you. The text on page X has been expanded as follows:

“Raised cholesterol was defined using a threshold of 5.0 mmol/l irrespective of whether respondents were currently taking lipid-lowering medication. This definition is consistent with usual Health Survey for England reporting which in turn reflects National Institute of Health and Clinical Excellence (NICE) guidelines [16].”

2) Measure of socioeconomic position (SEP):
Page 6 – The authors use the full index of Multiple Deprivation which, as they report, includes a health domain (years of potential life lost, Comparative Illness and Disability Ratio, Measures of acute morbidity and proportion of adults under 60 suffering from mood or anxiety disorders). This results in a circular argument as health is in both sides of the equation. It is best practice to exclude the health domain from the indicator when the outcome of interest is health related.

The main reason why the authors use an aggregate measure of SEP rather than the individual level (available in the study and their level of interest), is because these analysis are part of a larger project for which individual level will not be available for other outcomes. This is debatable given that this is submitted as an independent piece of research. I would even argue that carrying these analyses with the appropriate individual level of SEP may actually provide valuable information for the larger project because they will have an estimate of how much of the inequalities are underestimated because of the inevitable use of aggregate measures in the other CVD outcomes.

Thank you. In the discussion section we reference a UK study which has shown that excluding the health domain from the IMD measure had little impact on the results. We also point out that individual-based measures are also vulnerable to the circular argument. Our amended text on Page 23 is as follows:

“The IMD 2007 measure includes health-related data risking “mathematical coupling” [50]. However, a UK study has shown that removing the health domain from the overall index had
little effect on categorisation of areas or the strength of relationship between area-based deprivation and health [50]. Furthermore, such “mathematical coupling” can also occur with individual-based markers such as occupation or income being a consequence as well as cause in any association with health [33].”

In the discussion section we outline the further merits of using an area-based rather than individual measure of socioeconomic position. The text on Pages 22-23 is as follows:

“We chose IMD, a well-established marker of assigning socioeconomic circumstances based on area of residence for two main reasons. Firstly studies continue to show contextual associations between neighbourhood and health even after controlling for individual-level markers [46]. Residential deprivation is powerfully linked to health due to the influence of both composition (characteristics of individuals who live there) and context (features of the location itself) [47]. Area-based measures therefore may contribute additional socioeconomic information over and above that obtained from individual-level measures. Secondly area-based measures are particularly useful proxy measures of individual social position among younger or older age groups [48-49].”

3) Analysis:

I am not clear from how the IMD quintiles are entered in the model, as an ordinal or as a categorical variable. In order to report the inequalities in each risk factor the authors calculate a Relative Index of Inequality (RII) and then test whether this changed between the 90s and the 2000s. I believe the calculation of this RII is unnecessary in this case. Given that the authors are using quintiles of deprivation, a simple ordinal variable could have been used to summarize, in one value, the change in risk factor by deprivation level (although whether an ordinal variable provides a good description needs to be tested, more on this below). The RII is useful when comparing unequal groups as it weights for the different proportions of population in each group. However, by definition quintiles have equal proportions in each group. Furthermore, the RII, as an ordinal variable assumes linearity across groups. Thus, categorising deprivation quintiles in a 5 ordinal level variable should give to the same results and would appear more straightforward analysis. The authors should test though, whether the linearity assumption holds and otherwise analyse quintiles of deprivation as a categorical variable (but the RII is not a solution to this as the assumption of linearity is also true for the RII). I am also not clear why there is a need to test the 90s versus the 2000s, given that annual changes are available (unless there is an a priori reason why risk factors trends should be different between the two decades rather than change continuously over time). Whether deprivation changes over time is captured in the interaction test between quintiles of deprivation (ordinal if linearity holds or as a categorical variable otherwise) with year of survey. If I understand it correctly, this is the main analysis the authors have already done, therefore I am clear why there is a need of grouping years into decades. It is important to report this interaction though, because as it is in Tables 1 and 2, the authors provide a p-value (for the trend) within each quintile but these do not report whether there is a difference in trend by quintiles of deprivation.

Thank you. We have revised our methodology accordingly (set out in the Statistical Methods section on Pages 9-12). In brief:
The RII is no longer used as a summary measure of health inequalities.

We present annual data rather than divide the Health Survey data into two discrete time periods.

We use IMD in the modelling in the ways suggested above: i.e. as an ordinal level variable accompanied by tests of linearity (also see comments by Reviewer 2) or as a categorical variable otherwise.

We use the IMD × survey year interaction term as suggested above to test whether a unit increase in IMD on risk factor prevalence changes over time.

Presenting the results for IMD as a categorical variable is also useful for readers to understand the direction of socioeconomic gradients. Our amended text for the discussion of absolute inequalities is as follows:

“Linear regression models were used to estimate absolute differences in risk factor prevalence with the risk factor as the dependent variable and IMD and survey year as the two independent variables. Three models were fitted. First, four indicator variables for IMD were used with the most affluent quintile (Q1) selected to act as the reference category (Model 1a). The four coefficients denoted the difference in prevalence between each quintile and Q1 (year-adjusted). To present a more parsimonious model in the event of a linear relationship, we fitted an alternative model using IMD as a five category ordinal level variable ranging from 1 to 5 (Model 2a). The coefficient for IMD denoted the difference in prevalence for a one level (unit) increase in IMD quintile (year-adjusted). Using a linear term means that a unit increase produces the same absolute difference in prevalence regardless of where that unit increase occurs along the five category ordinal scale. The p-value served as a test for linear trend (5% as the threshold for statistical significance).

We assessed change in absolute inequalities over time using the significance level of the coefficient(s) for an interaction term $IMD \times survey\ year$, which was added to the model including IMD and survey year as independent variables (Model 3a). IMD was represented by an ordinal level variable if Model 2a showed supportive evidence of a linear trend. In this case, the interaction was represented in the model by a single term. Four indicator variables represented IMD if Model 2a did not show a linear trend. In this case, an overall test of four terms was used to examine whether the absolute changes in prevalence between each quintile and Q1 were all jointly equal to zero (i.e. no trend interaction effects).”

**4) Reporting of results**

I am of the opinion that trends are best shown with a graph. The authors do provide these but as supplementary material. I would suggest to bring these to the main paper and provide the yearly and quintile data in supplementary tables.

Thank you. We have taken this very good advice. The graphs of risk factor change have been brought to the main paper (Figures 1–4) with measures of absolute and annual change moved to supplementary data (Additional files 4–9).